Remarks on the Report of the Sanitary Commissioner with the Government of India on the Cholera Epidemic of 1872 in Northern India, by Surgeon-Major A. C. C. DeRenzy, B. A., T. C. D.

In the Report on the cholera epidemic of 1872 in Northern India, published in the ninth Annual Report of the Sanitary Commissioner with the Government of India, the writer, Dr. J. M. Cuningham, arrives at the conclusion that cholera is not spread by human intercourse, "and that it would be a gross exaggeration, as it is a pure assumption, to affirm that the troops and other communities who were attacked suffered because they drank water which had been contaminated by cholera discharges" (para 89). The belief in the dissemination of cholera through water is, he says, founded "on bare assertions altogether unsubstantiated by details, bare assertions such as would not be received by any Judicial Court, even in the pettiest case that could be brought before it." He declares (para 118) that as cholera enquiry is "on the wrong road, the sooner we confess that we are still in the same state of ignorance as we were in a hundred years ago, the better." These startling propositions coming from one in the author's high and responsible position have naturally excited considerable interest in England.

As the Report is to a great extent devoted to a refutation of certain views I have published from time to time within the last five years, I have long wished to offer some explanations in vindication of my opinions, but until the present moment I have not had the opportunity of doing so.

It appears to me that the proposition, that cholera is spread by human intercourse, is so fully proved that it would be superfluous to adduce any further evidence in support of it. I shall not therefore touch on this point any further than to show that the facts recorded by Dr. Cuningham do not warrant the conclusion he

draws from them. As regards the dissemination of cholera through water, I hope to show that the facts quoted in no way invalidate the opinion entertained on this subject in England, and that there are other facts which strongly corroborate it.

Dr. Cuningham rightly states that the questions at issue can only be solved by a patient investigation of facts, and he professes, in his investigation of the epidemic under consideration, to have enquired into "*every* fact which suggested itself as of importance, and to have recorded all the facts which seemed to be of any importance."

It might have been expected that, after so sound an appreciation of the proper method to be pursued in investigating cholera epidemics, judicial accuracy would be found in the summing up of the evidence, but it will be seen that this expectation is not fulfilled.

In the first place I must point out that the Report conveys a very erroneous impression regarding the extent of the field of enquiry. An ordinary reader would naturally suppose from the language used that the conclusions expressed were based upon facts elicited in a field of unexampled scope and breadth. It is twice stated that 108 different outbreaks were enquired into. "As they (the notes) relate to 108 different outbreaks, the facts recorded afford a fair basis for forming conclusions." Now on referring to the notes I find that at least two of the outbreaks were, so to speak, no outbreaks at all. The notes of outbreak No. 84 refer to the military station of Naushehra; but although there was no cholera there, but only "suspicious cases of fever, with vomiting, purging, and much depression as there are every year," the case forms one of the 108 outbreaks on the facts of which the conclusions arrived at are based. Outbreak No. 67 is of the same kind. The Medical Officer says there was "no case of cholera" in the Lahore Female Jail, yet it too

appears as one of the 108. No less than 26 out of the so-called *outbreaks* consisted of no more than three cases; several of them indeed consisted of a single case.

The total number of cases of cholera embraced within the scope of Doctor Cuningham's enquiry, about which information of any kind was obtainable, did not exceed 1,400, including in this estimate *every* case among the European troops, the Native troops and prisoners; and these 1,400 cases were distributed over an area upwards of 1,000 miles long and as many broad. In a vast majority of the cases little was known about them beyond the date of their occurrence.

Comparing these figures with the facts on which the conclusions contested by Dr. Cuningham are founded, how narrow appears the basis of his induction. In the London epidemic of 1854, the circumstances of 4,059 deaths were enquired into, and the London epidemic of 1866, which killed 5,973 persons, was enquired into with a thoroughness altogether unattainable in a country like India, where skilled medical observers are so few and far between in the midst of teeming millions of an ignorant, superstitious people. "No country in the world," says Dr. Cuningham, "presents such a field for the study of cholera as India." The field is indeed immense, the mortality caused by the disease is awful, but the difficulties in the way of studying it are commensurately vast.

As our knowledge of the diseases that prevail among the native population is inconceivably meagre, all enquiries into the origin of cholera or any other disease among the European troops must necessarily be inconclusive. The vastness of our ignorance of the state of cholera among the native population is well shown by this one fact. Of 165,000 deaths registered in 1872 there was not a scrap of information obtainable regarding more than at the very outside 2,000 of the whole number.

And of even the majority of the so-called outbreaks amongst the troops, the information available is so meagre as to be practically useless, as the following illustrations will show :—

No. 99, Shabkadar.

Details of the outbreak.—At Shabkadar, which lies 20 miles north of Pesháwar, a sowár was attacked with cholera on the 8th May. On the 14th September another sowár was attacked here, and three days afterwards there was a very suspicious case, which presented many of the symptoms of cholera. The Medical Officer, however, believes that it was one of Pesháwar fever.

Importation and communication.—There is no history of importation or contagion in any of these cases. None of the attendants were attacked.

No. 100, Abúzaí.

Details of the outbreak.—At Abúzaí, which is situate eight miles further on, a sowár was attacked on the 22nd October, the only case within this Fort.

Importation and communication.—It could not be traced to importation.

No. 101, Cherát.

Details of the outbreak.—On the 25th October a Private of the 55th Regiment was attacked with cholera; he recovered.

Importation and communication.—His seizure was attributed to indulgence in fruit. It could not be explained by importation, nor did any other attacks follow.

The above verbatim extracts are fair specimens of "the details of outbreaks" in nine-tenths of the cases recorded in the Report. It will be observed that the diagnosis of cholera is not altogether free from doubt and difficulty, as the disease in many instances closely

resembles remittent fever attended with an aggravated cold stage. The Shabkadar cases all recovered. The case at Abúzaí proved fatal. It occurred in a sowár, the nature of whose duties took him from time to time into Pesháwar and the vicinity where cholera was epidemic at the time he was attacked. Though it could not be traced to importation, the circumstances were very far from excluding importation or rendering it improbable.

As regards the fulness and exhaustiveness of the enquiry also, the Report conveys a very erroneous impression. It would be supposed that all the usually recognised media for the transmission of cholera had been the subjects of investigation. Such an impression would be entirely wrong. Milk has of late years been recognised as a very common medium of infection by the specific viruses. But in the Panjáb portion of the enquiry, at which I had the opportunity of being present, I can affirm that not a single question was put on this subject, and the question of infection through food generally was entirely ignored. Not that its importance was not fully understood, but it was very properly taken for granted that no information on the subject of any value whatever was available.

I have now, however, to call attention to an error of a more serious nature than the not unnatural exaggeration of the value of the opportunities for the study of the etiology of cholera afforded by India.

Dr. Cuningham lays great stress upon the history of the outbreak in St. Peter's College, Agra, as proving that cholera is not spread by human intercourse. In para 59 he says—"On the 6th July 65 of the boys were sent to their friends either in Agra or other stations, some of them very distant. If cholera be a disease which is spread by human intercourse, nothing could have been more favorable for its propagation.

There can be no question that the cause existed in a most virulent form at the College, and yet in no instance did one of the 65 boys so dispersed over the country communicate the disease to the homes into which they were received." If the facts were really as here stated, it would, indeed, be difficult to resist the conclusion drawn from them. But it will hardly be credited that in a matter of such vital importance the evidence has been altogether misrepresented. Yet such is the case. Dr. Cuningham had no authority whatever for saying that "in no instance did one of the 65 boys communicate the disease to their homes," for he knew nothing whatever of the history of 12 of the number. Here is the proof of this assertion. At page 74 of the Report we read—"Father Symphorien, the resident head of the institution, has been kind enough to furnish me with a list of all the boys who were sent away from St. Peter's on the 6th July on account of the outbreak. *Some of them cannot be traced,* * as their friends have changed their residence, but the following particulars have been ascertained regarding 53 out of the 65 who were sent away on the 6th"; 53 of the boys who were sent away not having communicated the disease to their families, Dr. Cuningham thought himself warranted in saying that as nothing was known of the remaining 12, they also had not infected their families. It is certainly very remarkable that in a country where the movements of Europeans with families are traced with unusual facility, 12 of the 65 boys who left St. Peter's College should, in three or four months, have so entirely disappeared that no trace of them, or of their parents or friends, could be found. The managers of a school, in which 34 out of 176 children were killed in a few days by cholera, (*vide* page 73) would be more than human if they resisted the temptation of shifting from their own shoulders the responsibility for this

* The italics are mine.—A. C. C. DeR.

awful catastrophe. General custom sanctions the practice of throwing the blame of such occurrences on Providence, and this appears to have been the course followed in the present instance. No human being was to blame for it; it was nobody's fault. There were no grounds, indeed, for supposing that the use of water taken from a well which drained a latrine that contained the excremental accumulations of years had anything to do with the outbreak. "The well had ceased to be used for supplying the boys." It was kept open, however, all the same, and must have been used for some purpose or other. With this well on the College premises, we are asked to believe that the water-supply was entirely carried in leather bags from a well a third of a mile distant. Dr. Cuningham apparently credits this statement. It was with great difficulty that Mr. Netten Radcliffe induced the old carpenter, who had the unfiltered Lea water pumped over East London in 1866, to admit that he had done so. If Dr. Cuningham had shown something of Mr. Radcliffe's wholesome scepticism in investigating the St. Peter's College outbreak, the details elicited would, I believe, have been very different from those recorded in the Report. At all events there are no grounds whatever for the assertion that in no instance did one of the 65 boys removed from the College communicate the disease to their families. Until something is known of the 12 who could not be traced, such a statement would be unwarrantable. Loose assumption and inaccurate statement of the kind now referred to run through the whole Report, and form the sole basis for the conclusion that cholera is not communicated by human intercourse. Because the first cases that came under the observations of European Medical Officers could not be traced to importation by human agency, the inference is drawn that they were not imported. And here I have to note that in several instances it is not correct to say that the

first cases *could* not be traced to importation, but that they *were* not traced, and for the best of reasons, namely, that the Medical Officers made no attempt to trace them, as they either had no leisure or thought it useless to attempt to do so in the presence of the almost insurmountable difficulties which surround such enquiries in this country. I conceive that in summing up the evidence Dr. Cuningham was bound, as an impartial judge, to have stated this fact, but no mention whatever is made of it in either the body of the Report or the notes attached to it.

The statement made in para. 58 regarding the influence of Railways in hastening the diffusion of cholera betrays the usual one-sidedness in the record of evidence. The following figures for Mián Mír are extracted from a table showing the dates of previous epedemics in certain stations :—

Station.	Years.	Strength.	Admission from Cholera.											
			January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Total.
Mián Mír ...	1856	1,576	...	...	...	...	...	...	...	435	65	1	...	...	501
	1861	1,700	...	...	...	...	...	...	...	661	64	...	...	...	725
	1869	974	...	...	...	...	...	...	...	...	...	...	...	...	...
	1872	1,358	...	...	...	..	...	...	*1	179	5	...	...	...	185

* On the last day of July.

The following comment on the table is made :—"A glance at these figures will show that cases occur no earlier than they did before. At Mián Mír it is worthy of notice that in the epidemic of 1861 the first case among the Europeans was on the 2nd August. In 1872 it was on 31st July, a difference of two days."

In order to make the table square with the comment, it was necessary to omit from it the details of the

epidemic of 1867, which *began in May*, and attained its maximum in July, and in which there were 86 cases altogether and 52 deaths. This epidemic does not appear in the table at all, though the influence of railway communication in diffusing it was most marked. It was also necessary in discussing the influence of Railways on the rapidity of cholera diffusion to omit to mention the fact that in 1872 cholera appeared in the month *of May*, unusually early, and became most virulent in June in the Lunatic Asylum at Lahore, five miles north of Mián Mír. Facts like these need no comment.

There are two surprising mis-statements in para. 57. "The fact that the well elevated hill stations," it is said, "almost always escape (cholera), and the exceptions to this rule in 1872 are alike inexplicable on the theory of human intercourse." After such a statement it will surprise the reader to find that Murree, the most elevated of all the hill stations, has suffered terribly in three out of the eight epidemics which have visited the Panjáb since annexation. There are, in fact, few Panjáb stations which have suffered more frequently or more severely. At page 122 it is stated that "the Murree Depôt has suffered twice before from cholera, once in 1858 and again, to a slight extent, in 1867." It will be observed that in giving the previous cholera history of Murree, the statement is restricted to the history of the depôt, apparently for the purpose of ignoring the terrible sufferings of the native population of the sanitarium, in the midst of which the depôt stands. A reference to page 109 of the Panjáb Sanitary Report for 1868 will show that the epidemic of 1867 killed no less than 90 Natives and 11 Europeans out of an aggregate population of less than 11,000 souls. At page 65 of Dr. Cuningham's own Report for 1867 it is stated that "Murree suffered to a considerable extent, and the presence

of the disease which was spread over many weeks caused much anxiety among the residents. Many natives, servants belonging to the visitors, were attacked in the various compounds. 46 cases were treated in the cholera hospital, of whom 24 died, 12 Europeans were seized, and of them 9 died; the type of cholera is reported to have been most malignant." This is the epidemic which in 1873 Dr. Cuningham describes as "slight." In 1867 he had not broached his theory of "localities," and his mind being unwarped by preconceived opinions, he was able to describe events accurately and impartially; in his later Reports he has lost this invaluable faculty. In para. 57 it is also stated that "with the single exception of 1867, when the disease was chiefly confined to the native servants, the oldest inhabitant cannot remember the occurrence of cholera at Simla, excepting in a few isolated cases, most of which were imported." A reference to page 140 of the Panjáb Sanitary Report for 1868 will show how fallacious is the memory of the oldest inhabitant of Simla. It will there be seen that in 1857 there was an epidemic of cholera at Simla which killed five Europeans out of the very small Protestant community. It is impossible to say how many deaths occurred among Catholics, nor is the total number of cases ascertainable. In 1867 six Europeans of all classes fell victims to the disease. As no mortuary records were kept in 1857, there is the same ignorance regarding the number of cholera deaths and cases among the native population, but there is little doubt that both were considerable.

At para 119 it is stated that "it is the doctrine of contagion which has prevented progress in sanitary improvement." This statement must surely have astonished those who are aware of the fact that it is only within the last 10 years that the doctrine of the contagiousness of cholera has been at all admitted in this

country, and also those who are aware of the wonderful effects which the recognition of the contagiousness of typhus has had in limiting the ravages of that disease in Ireland. There are few well-informed persons I imagine who will dissent from Dr. Parkes's dictum that the doctrine of the dissemination of some of the specific diseases by drinking water is "certainly one of the most important steps in etiology which has been made in this century." To that doctrine we are mainly indebted for the great improvements in water-supply which have been made of late years in London and other English towns. Nothing short of the irresistible cogency of Dr. Snow's demonstration could have overcome the inertia of the great London Water Companies, and compelled them, in the first instance, to change the situation of their in-take, and subsequently to filter their supplies. In proof, however, that sanitary improvements in no way, based on the doctrine of contagion, are efficacious in preventing cholera, Dr. Cuningham refers to the case of the prisons of Upper India. At para. 120 he says :—"We know by experience that much may be done by sanitary improvements to affect the incidence of cholera in particular localities and to diminish its violence. Of this there could be no more striking example than the history of the jails in Upper India." And he goes on to show the undoubtedly great reduction of cholera mortality in these institutions. From this illustration it would naturally be supposed that the sanitary state of the Jails was at least tolerably satisfactory, and that the prisoners confined in them enjoyed a fairly good standard of health. How erroneous such an inference would be will appear from the following figures extracted from the Jail Report of the North-Western Provinces for 1873, and showing the death-rate per mille of six of the Jails for the two years 1872 and 1873, as well as the general death-rate of the entire prison population of the province :—

	1872	1873.
Meerut Central Prison	121·3.	76·7
Do. District Jail	126·7	105·5
Bandá	7·12	106·3
Bareilly Central Prison ...	55·5	87·2
Sháhjahánpúr	30·8	87·7
Mirzápúr	5·53	81·4
Death-rate of the whole prison population	4·88	4·8

This table tells its own tale of the sanitary arrangements to which Doctor Cuningham attributes the comparative immunity of the prisons from cholera. I will only remark that the years quoted are not at all exceptional ones. Hardly a year passes in which the convicts of one or more prisons are not literally decimated by epidemic diseases other than cholera, and death-rates of from 12 to 15 per cent. (per cent. I repeat) are not at all unfrequent. Bad as is the state of things among the European troops, death-rates, like those shown in the table, have long ceased to occur, except in connection with cholera, and yet there is no denying that the troops are far more subject to that disease than the convicts, and the latter suffer far less from it than they used to do formerly. After what has already been stated no one will attribute the immunity of the convicts to the excellence of the prison sanitary arrangements. What then is the diminished liability of prisons to cholera due to? The answer I would give is that it is due to the growth of the doctrine of the communicability of cholera in Upper India,—a doctrine for which we are mainly indebted to the indefatigable Surgeon-General John Murray. Under the old regime of non-contagions, no endeavours were made to exclude the contagions, and frequent outbursts of pestilence in the prisons were the result. For some years the doctrine of the communicability of cholera in some form or other from person to person has been generally accepted in Upper India, and the natural corollary of the doctrine has been acted

on, namely, the expediency of segregating the prisoners as much as possible from infected populations, by restricting communications, keeping a close watch on newly-convicted prisoners, and scrutinizing the sources of food supplies. These precautions are still far from being as strictly enforced as they might be, but, however imperfectly observed, I believe they have sensibly lessened the chances of cholera contagion being imported into the prisons of Northern India.

The following extract from para. 320 of the Report of the Sanitary Commissioner with the Government of India for 1867 shows that at that time the explanation here given of the immunity of the prisons was admitted by no less an authority than Dr. Cuningham himself :—

"Practical results are of much greater value than theoretical opinions, and the introduction of even such an imperfect quarantine as has been practicable in the Jails of the Upper Provinces has been attended with most excellent results."

There is no mention here of the immunity being due to Sanitary improvements, for the writer of the Report will know that no such improvements had been made.

And here I feel bound to state that, fully convinced as I am of the value of segregating prisoners, I am in no way responsible for the impotent attempts at quarantining towns and villages, and even districts as large as Yorkshire, which Dr. Cuningham so strongly but justly condemns. It is only fair, however, to the local authorities who made these attempts to state that they were only carrying out instructions which had been originally suggested by Dr. Cuningham himself in 1867 when acting as Secretary to Colonel Malleson, then Sanitary Commissioner with the Government of India.

In dealing with the questions of the diffusion of cholera by water, Dr. Cuningham shows the same strong bias in stating and in suppressing facts, and the same incapacity to appreciate the value of evidence, as he has exhibited in summing up the evidence on the question of contagion. "All the facts," he says, "which seemed to be of any importance, have been entered." The three following illustrations will show how this promise has been fulfilled :—

(*a.*) At Jálandhar the epidemic fell with marked severity on the married families. Struck with this fact, I examined a specimen of the water in use in the barrack at the time of Dr. Cuningham's enquiry, and, finding it quite milky in appearance, I showed it to him. Enquiring afterwards into the cause of the milkiness, I found that the water was taken from a well situated close to a sandy surface drain into which the drainage of a wash-house and latrine and urinal discharged. Owing to the sandiness of the soil, the drainage was all absorbed on the spot, and, no doubt, percolated back into the well, which was only about 45 feet from the drain. The married families were very unhealthy at the time of inspection, suffering much from fever and diarrhœa ; the rest of the Regiment being tolerably healthy. Her Majesty's 54th Regiment, then in occupation of the lines, as well as their predecessors, had suffered much from enteric fever. These facts were all brought to Dr. Cuningham's knowledge, but the only notice of them taken in the Report is the following—(page 96):—"The water-supply is from wells ; of these there are seven in the 54th Lines. The position of these wells close to buildings and surface drains, in some cases skirting their immediate vicinity, is objectionable. There is no evidence that the occupants of barracks supplied from any particular well suffered more than others. There is no appreciable difference in the

sanitary condition of the Lines occupied by the 54th and of those occupied by the Artillery."

(*b*). Page 115.—In describing the water-supply arrangements of Her Majesty's 37th Regiment, in which there were 82 deaths from cholera, it is stated—"They (Regiment) were provided with Macnamara filters; the whole Regiment drank from them, except the few men on guard." Now the Macnamara filter is considered a very effective one. Doctor Macnamara has recently received from Government a very handsome bonus for his services in inventing it, and it was said and believed by many persons that the general adoption of the Macnamara filter was all that was necessary to ensure the supply of pure water to the troops. It will be seen then that the impression conveyed by the passage quoted is that all necessary precautions had been taken against the supply of impure water to Her Majesty's 37th Regiment, and that in that case at all events the Regiment could not have been infected through the water-supply. At the close of the enquiry I asked to be allowed to see one of the Macnamara filters, and on drawing some water I was surprised to find it quite opalescent. I then asked to have the interior of the filter examined. I should state that Doctor Macnamara's arrangement is only a slight modification of the well-known Danchell filter, the filtering medium being animal charcoal. On the contents of the filter being emptied out, there was not at first the slightest appearance of charcoal; the contents appeared to be a homogeneous mass of mud, and it was not until a thick envelope of mud had been washed off the nodules of charcoal, that any trace of the original filtering medium became at all visible. So entirely was the charcoal concealed by the mud that it was for a moment doubted whether there was any at all of the former substance present in the vessel. This discovery was made in Dr. Cuningham's presence, but

the fact does not appear to have had sufficient importance in his eyes to deserve mention in his notes. On making further enquiries in Dr. Cuningham's presence, I elicited the fact that no one knew any thing of the state of the filters, and that whatever was said on the subject was the merest guess-work.

(*c*). In the Panjáb Sanitary Report for 1869 I had for the first time called attention to the remarkable fact that the Fort of Pesháwar had passed almost unscathed through the terrible epidemics which had visited that station, and that this immunity was the more remarkable, because the Fort was extremely unhealthy. I attributed the circumstance to the fact that the Fort was supplied with water from a well which, though an extremely bad one, was more safe from choleraic contamination than the road-side gutter water so generally used in the Cantonment. On visiting Pesháwar in company with Dr. Cuningham for the purpose of being present at his enquiry into the epidemic of 1872, I was surprised to find that the European troops in the Fort had suffered with special severity, and for a time I was compelled to doubt the correctness of my own explanation. But casually, and after some days, the novelty of an epidemic in the Fort became less inexplicable. It appeared that about *a week before the appearance of cholera at Pesháwar* the Medical Officer of the Fort, seeing how bad was the Fort water, and how much the troops were suffering from fever, and not knowing any thing of what I had written about the immunity of the Fort from cholera, had recommended that Cantonment water should be supplied, and, in accordance with this recommendation, Cantonment water was supplied and sent in casks, the distance between the Fort and Cantonment being about two miles. The water was supposed to be taken, as ordered, from a good well known as Mackeson's; but Doctor Cuningham's Report

(page 129) shows that in one case at least the water-carriers, to save themselves the trouble of lifting water by means of ropes and pully blocks from a depth of 90 feet, had filled their vessels from the roadside gutters. Whether they did so or not in the case under consideration is unknown, but the fact remains that in 1872, for the first time in the history of the Fort, the European troops were supplied with Cantonment water, and that this was also the first occasion of their being affected with cholera. The native portion of the garrison continued to use the well water as formerly, and enjoyed their former immunity from the disease. How are these facts represented in Dr. Cuningham's Report? Contrary to the express opinion of the Medical Officer in-charge (page 132), he represents six cases of fever which occurred on the 1st October, before the change of the water-supply was made, as cases of cholera. One of the six cases died 10 hours after admission to hospital with symptoms not unlike those of cholera, but which are known to be extremely common in cases of Pesháwar fever; the other five did well. The six cases in question occurred eight days before any other European was attacked at Pesháwar, and no other cases of cholera or of a choleraic character occurred in the Detachment during their subsequent stay of six days in the Fort. That there may be no doubt of the opinion of the Medical Officer under whose charge the fatal case referred to occurred, I quote his evidence as recorded at page 127 :—

"Dr. Triphook is still of opinion that it was not one of cholera. He states that before and during the outbreak of cholera he saw a number of cases of malarial fever which were attended with bilious vomiting, diarrhœa, and prostration in a marked degree; in one case in particular, which occurred during the time of the cholera, the collapse was so severe that I apprehended the worst." And yet, notwithstanding this, the follow-

ing passage occurs at page 132 :—" The cases described as having occurred on the 1st of October were attributed to the water, which was then drawn from a well near the Fort, which is especially liable to contamination. From the 7th the water was supplied from Mackeson's well in Cantonments, and sent down in barrels daily." Here all the six cases are assumed on no evidence whatever, or rather against all evidence, to be cases of cholera. This is what Dr. Cuningham calls "summing up the evidence."

Dr. Cuningham speaks with the utmost confidence of the value of the facts he has recorded on the subject of water-supply (para. 89). "The facts recorded under this section of the notes," he says, "are not to be ignored or slurred over. If the water theory is to be upheld, it is not sufficient to refer to the experience of East London or the Broad Street Pump. If it is true, its truth must be generally applicable." So irresistible he considers the cogency of his facts that he, in effect, calls upon English Epidemiologists to surrender at discretion, and to abandon views which it took 17 years of careful research to establish. I cannot but think, however, that if Dr. Cuningham would only study his own Reports from 1867 to the present time, his estimate of the value of his "facts" would be considerably lowered. In reporting on the great Hardwár epidemic of that year, which killed 479 soldiers, he appears not to have had the smallest suspicion that the state of the water-supply of Military Cantonments was in any way answerable for the terrible sufferings of the troops. I have failed to find in the Report any allusion whatever to this subject. He appears to have shared in the general opinion of those days that the water-supply was good, and that no fault could be found with it. In the Army Medical Report for 1866, Appendix No. 43, the Bengal Statistical Officer, when treating of the Hardwár epidemic in Cantonments, expressly states that

"in none of the Reports are there any strictures reflecting on the quality of the water drunk by the troops." Even in describing the fearful epidemic of 1867 at Pesháwar, which nearly decimated the troops, Dr. Cuningham does not make one syllable of reference to the state of the water-supply, which every one now admits to be a public scandal; for anything that appears to the contrary, the water-supply of that station might be considered every thing that is desirable. Nor in discussing the preventive measures suggested by that epidemic does he make the slightest mention of the state of the water-supply. The only measures he thought advisable in those days were the quarantine of Cantonments, so long as they remained free from cholera infection, and the movement of the troops into camp, in cases where the Cantonments became infected. In his Report of the epidemic of 1869, the "facts" have changed slightly. In three Panjáb stations, Amritsar, Kohát, and Pesháwar, the water was announced (para. 159) "to be notoriously open to pollution," but (para. 163) "there is no evidence to show that the water was the cause of the disease." In other stations the water is usually reported as "good" or "excellent." At Sabáthú, we are told, the water was "wholesome and not liable to contamination" (para. 146). In 1872 the "facts" have again changed. It is now declared that "no one will deny that the water in many of the places concerned is liable to pollution" (para. 89), and even at Sabáthú, whose water in 1870 was "not liable to contamination," the following suggestion for preventing contamination was deemed necessary:—"The reservoirs should be raised so as to prevent the possibility of any surface drainage entering them" (page 82). And at Kasauli "there is every opportunity for a naturally excellent water to become contaminated" (page 86). But still it is confidently asserted that "it is a gross exaggeration, as it is a pure assumption to affirm that the troops suffered

because they drank contaminated water." * * To erect any hypothesis upon the shifting facts furnished by Dr. Cuningham would be as futile as to build a house upon sand.

I have said enough, I think, to show that Dr. Cuningham is not an impartial judge on the subjects discussed in his Report. I could show, were it necessary that he has admitted evidence as to facts which were evidently beyond the cognizance of the witnesses, in truth the merest hearsay, that he speaks of there being no evidence of importation of cholera when the witnesses admitted that owing to press of work no such evidence had been sought for, and that almost universally the evidence tendered regarding the connection between cholera and the use of impure water was of the vaguest and most conjectural character. Water was said to be good, which was found on actual examination to emit a strong musty smell, and to have either a brown or almost a milky white color. Although in paras. 32 and 33 so much stress is laid on the difference between facts and opinions, a very cursory examination of the Report suffices to show that, notwithstanding that the latter are stated not to be recorded, they, in truth, form the great bulk of what is recorded in the Notes under the head of "Importation and Communication."

There are remarks made in paras. 117 and 222 which deserve a little notice. They illustrate the peculiar views held by Dr. Cuningham on the subject of the causation of disease generally, and go far to explain the extraordinary position he has taken up in reference to the question of contagion. He says "do we know that small-pox spreads only by contagion? Its annual rise and fall in this country recurring steadily year after year, its comparative dormancy for a term of years, and then a year of epidemic violence, are facts altogether

inexplicable on the doctrine of contagion, and prove incontestably that the law of contagion is not the law which governs the spread of small-pox." At para 222, speaking of the advancement of sanitary reform he says: "this is to be attained not by impressing on the people the necessity for quarantine or for isolation of the sick or for disinfection or for any other measures directed specially against contagion, which even if they could be shown to be based on sound theoretical reasoning, can never be carried out in practice."

From these passages it is obvious that because small-pox epidemics evince a marked tendency to periodicity, because for some unknown reason they recur in cycles, and are specially fatal in certain months of the year, Dr. Cuningham doubts the contagiousness of small-pox, and questions the advantage of measures of segregation and disinfection as means for preventing that disease. Many persons will think it a waste of time to controvert such views. I only refer to them for the purpose of calling attention to Dr. Cuningham's peculiar opinions on the subject of disease causation generally. It is quite evident that he uses the term *cause* throughout his report in an unusual sense. "In common language, says Professor Bain (Logic Induction page 17), the cause of an event is some one circumstance selected from the assemblage of conditions as being practically the turning point at the moment, what is in our power and needs our attention." It is in this sense that contagion is said to be the cause of an attack or of an epidemic of small-pox, for by segregating or by destroying the contagion the extension of the disease is prevented. Contagion most certainly does not explain *all* the phenomena of small-pox epidemics. There are other still unknown conditions which are perhaps quite as essential as contagion to the production of an epidemic, but as contagion also is essential, and as by operating upon it we can prevent

the small-pox epidemics, we are perfectly justified in speaking of it as *the cause* of the epidemics, using the term in its practical sense, the one always used in medicine. Dr. Cunningham however, manifestly uses the term in its scientific sense in which the "cause must be regarded as the *entire aggregate* of conditions or circumstances requisite to the effect." In this sense the cause of every disease is unknown, and will probably remain unknown for ever.

I shall now state very briefly my own views on the question of cholera enquiry generally in India. My belief is that the conditions under which cholera occurs in this country are so complex that no substantial increase in our knowledge of the etiology of the disease is to be looked for as the result of enquiries carried on here. Such enquiries are unavoidably from the nature of the case of the most superficial character. The little we know of the state of disease among the native population serves to show rather the vastness of our ignorance than the accuracy of our knowledge. For many generations to come it will be impossible to trace the course of cholera or other epidemics in this country with any sort of exhaustiveness, and all opinions regarding the origin of any particular epidemic, being due to importation or not, will deserve very little attention. Nor is there much hope of our being able to connect any particular epidemic with the use of tainted water by evidence of a direct specific character. If the population of London had been supplied with water from thirty thousand surface wells, Doctor Snow's superb demonstration would have been utterly impossible even with the Registrar General's returns in his hands. In a country like this where wells are scattered broadcast over the surface, all more or less exposed to contamination and yet not all contaminated, and where no one knows from how many wells he drinks water in the course of a single day, it is almost hopeless

to trace the source of an attack of cholera with any sort of conclusiveness to the use of any particular water. A soldier may get pure water in his barracks; seized with thirst when walking along the road with a thermometer standing at 100° F., he drinks greedily from the road gutter or from a foul well, or he gets tainted water mixed with his milk, or to see a friend he goes to another barrack, the water supply of which has been tainted by a man, affected say with the premonitory diarrhœa of cholera, putting his hand into the water jar; the water jar has a capacity of about three or four gallons, and into this small volume of water the soiled fingers and cups of from 20 to 30 men are being continually thrust to draw water. When off duty a man may drink one water, when on guard another, on parade a third, in the bázár a fourth,—almost infinite is the complexity of the conditions connected with the water-supply. It is this complexity which has obscured the causal nexus between disease and the use of foul water in India. A justly eminent medical writer giving the results of thirty years' observation in the Bombay Presidency states very erroneously, as will be seen hereafter, that "bad water has not been a common cause of disease." * This too is the reason why Dr. Cuningham, while admitting that good water "is of vast importance as a means for the prevention of cholera," and that in numbers of cases the present supply is bad, yet with a logic unintelligible to me denies that the existing state of the water-supply has any causal relation to the prevalence of cholera, enteric fever, or in fact any other disease whatever. The truth is that the relation in question is only recognisable in the comparative simplicity of the conditions in which cholera and enteric fever occur in England. But surely, bearing in mind the uniformity of nature's operations, we are justified in

* Report on Sanitary Improvements in India up to the month of June 1869, page 152.

arguing that the law of cholera dissemination in this country cannot be very different from what it is in England; and this *a priori* reasoning is beginning to be confirmed by facts of Indian origin, which seem incompatible with any other view than that the present state of the water supply is, I will not say the only factor in the dissemination of cholera, but one of infinitely more importance than any other or than all others put together. Drs. Bryden and Cuningham have naturalised the idea that a certain portion of the Gangetic valley is the endemic home of cholera, and that in all other parts of the peninsula the disease is only an epidemic exotic. On referring, however, to the history of cholera in Bombay, I find that from 1849, the earliest date to which mortuary registration extends back, there were, previous to 1866, only two years in which that city could be said to be free from the disease in an epidemic form. In proof of this statement I herewith annex a list of the deaths register from cholera in Bombay in each year from 1849 to 1873 :—

*	Year.	Deaths.	Year.	Deaths.
	1849	2,128	1862	3,170
	1850	2,997	1863	2,209
	1851	5,485	1864	4,847
	1852	1,520	1865	2,887
	1853	1,148	1866	332
	1854	3,507	1867	111
	1855	1,645	1868	227
	1856	1,846	1869	754
	1857	2,181	1870	386
	1858	115	1871	294
	1859	1,985	1872	191
	1860	1,961	1873	95
	1861	641	1st Quarter of 1874	4

These remarkable figures show that from 1866 to the present time, although cholera has been

* The figures for the years 1849 to 1870 are extracted from the proceedings of the Sanitary Commissioner with the Government of India for the months of November and December 1871, page 419, and those for the years from 1871 to 1874 are taken from the Bombay Health Officer's reports.

repeatedly epidemic in the Bombay Presidency, and although from the opening of railway communication the chances of the importation of the contagium into the city have been immensely increased, the disease has for a period of 8 years failed to assume epidemic proportions. In the streams of pilgrims, Muhammadan and Hindú, isolated cases of cholera have been repeatedly detected, but the disease has proved impotent to propagate itself among the population. What has caused this great change? Doctor Cuningham would answer sanitary improvements. But I enquire—What sanitary improvements? The city is incomparably cleaner than it used to be; still as regards cleanliness, using that word in its usual general signification, it cannot be compared with the cantonments of Upper India. I have gone on foot through every part of Bombay to satisfy myself on that point. It is not therefore to its cleanliness that it owes its immunity from cholera. Nor is it to its drainage, which I do not use too strong a term in designating as infamous. The sewers are filled with tons of deposit whose putrid emanations almost choke one in certain places. Nor is the immunity due to the superior ventilation of the streets, nor to improvements in house accommodation. The native town is crowded on a par with the densest portions of Liverpool or Glasgow, in this respect comparing most unfavorably with the scattered populations of the Upper Indian Cantonments. If the cessation of cholera in Bombay therefore can not be due to any of the conditions mentioned, what then can it be due to? In 1865 measures were taken to distribute the Vehar water over the city in iron pipes and to close numbers of surface wells; the new water was made more convenient for use than the old, and the people took to it readily. This I believe is the cause of the subsidence of cholera in Bombay. Other minor causes may have co-operated to some extent, but

the change in the water was the main one. In Colaba, a suburb of Bombay and a Military Cantonment, the troops previously to 1866 used to suffer constantly from cholera, dysentery and other epidemics. Colaba was the Pesháwar of the Bombay Presidency, and the Government were constantly urged to abandon the station altogether as being radically and incurably unhealtly. In 1865 the barracks were connected with the Vehar water pipes, and from that date the epidemics ceased. All the supposed causes of the unhealthiness of the station except the water had been remedied without appreciable effect. With the change in the water supply the station became permanently free from the old epidemics. Is it not a warrantable induction to conclude that the old state of the water supply was the cause of the epidemics? Even if this case stood by itself, the induction would, I think, be warranted. Taking it in connection with similar sequences of phenomena elsewhere, the conclusion becomes a moral certainty.

In Calcutta, also, very similar results have been obtained; the deaths from cholera have been reduced to less than one-third of what they were formerly, as will be seen by the following Table :—

Deaths registered from cholera in Calcutta.

1866	...	6,823	
1867	...	2,268	
1868	...	4,178	
1869	...	3,592	
1870	...	1,563	
1871	...	800	
1872	...	1,142	*

In that case some share of the reduction may be due to the improved sewerage which came into operation at about the same time with the issue of good water. But

* The new water supply became available in 1870. The figures are taken from page 108 of the Report on Sanitary Improvements in India for 1873, and page 140 of Ninth Report of Sanitary Commissioner with the Government of India.

knowing that, as in the case of Bombay, the worst conceivable sewerage may co-exist with marked immunity from epidemic cholera, the great bulk of the reduction of cholera mortality in Calcutta must be attributed to the new water supply. Considering how largely tank water is still used in that city and how large a proportion of its semi-migratory population are in constant communication with the cholera scourged villages in the vicinity, the decrease of the cholera death rate is quite as much as could reasonably be expected. In Bombay the results were more decisive, as from its insular position the communication between the town population and the adjacent village population is more restricted.

Dr. Cuningham having adopted strongly the old Indian opinion that bad water has not been a common cause of disease, has until lately steadily opposed any radical change in the water-supply of Cantonments. When consulted by Government in 1869 as to the changes that were necessary, this was the advice he gave in regard to 24 of the principal Cantonments, including unhealthy stations, like Allahábád and Lucknow, "Measures are required for preserving the natural purity of the water, and for this purpose the wells should be provided with a ridge wall, a platform and a drain to lead away waste water; they should also be periodically cleansed and to facilitate this operation the bottom should be tiled. In the lines of every European Regiment in India one pump should be fitted to the best well, the water should be raised by the pump, should be passed through a filtering bed attached to the well, and it should then be received into a metal pail, and thus be conveyed to the barrack filter. If these simple appliances are brought into use, nothing more seems to be necessary to render the water-supply all that can be needed." For Pesháwar he recommended that well water alone should be used, and that, if necessary, additional wells should be sunk.

It thus appears that Dr. Cuningham attributed very little importance to the systematic distribution of water over every portion of a Cantonment and for all classes of its population. One pump was to suffice for the lines of a European Regiment, usually covering an area of a square mile or two. The drinking water was to undergo a double filtration, and to be distributed by manual labor, metal pails being substituted for leather massaks. The supply of the native population was to be left in its present state. With such notions as to the nature of the reforms required in the water-supply of Indian Cantonments, Dr. Cuningham naturally resents the suggestion that the present state of the water-supply is the great source of Indian epidemics. When in 1870 the late Lord Mayo visited Pesháwar, he saw at once the absurdity of the measures proposed for that station, and ordered the immediate adoption of the comprehensive scheme that had been previously recommended by the Panjáb Government. Since Lord Mayo's death the scheme has hung fire, and, after five years' discussion, is as far from completion as ever. Dr. Cuningham's report has damped the zeal of the authorities in the matter of water-supply reform. While ready to assent to his formula that good water is a most desirable thing, they are not prepared to incur a large outlay in altering the existing arrangements, which the highest sanitary authority declares that "it is a pure assumption" to affirm to be in any way concerned in the causation of cholera, enteric fever, or the other diseases that impair the efficiency of the troops, and enhance the cost of the Army. I trust, however, that ere long the opinion of high sanitary authorities in England will be expressed on the points at issue with such clearness that Indian Governors will be encourged to proceed with confidence in the great work of placing the water-supply of Indian towns and Cantonments on a satisfactory footing.